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On Franklin & Electricity

REASONS

FOR DISSENTING FROM THE

REPORT OF THE COMMITTEE

APPOINTED TO CONSIDER OF

MR. WILSON'S EXPERIMENTS;

INCLUDING

REMARKS

ON SOME

Experiments exhibited by Mr. NAIRNE.

By SAMUEL MUSGRAVE, M. D. F. R. S.

Read at the ROYAL SOCIETY, June 25, 1778.

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REASONS, &c.

I DO not find that Dr. FRANKLIN, in any of the paſſages where he ſpeaks of the efficacy of ſharp-pointed conductors to prevent electrical exploſions, has expreſſed any doubt of their being univerſally preferable for this purpoſe to thoſe which have a blunt or ſpherical termination. The ſame obſervation may be made of the other gentlemen who are the advocates for his doctrine. It may therefore be aſſumed, that both he and they mean to aſſert an univerſal propoſition, "That ſharp "points will, in all caſes, draw off the electrical fluid "ſilently within the diſtance at which rounded ends "will explode; or, at leaſt, that the former ſort will in "no caſe receive an exploſion at a greater diſtance "than the latter." I think it neceſſary to obſerve, that, though I diſſent from this doctrine, I do not mean to aſſert the contrary univerſal propoſition, but only to deny the univerſality of that aſſerted by Dr. FRANKLIN, which

I apprehend to be ſometimes true, and ſometimes alſo falſe.

But before I attempt to ſpecify the particular caſes in which the ſharp and the blunt terminations are reſpectively more liable to electrical exploſions, it may be of uſe to ſhew (what many gentlemen ſeem not to be thoroughly aware of) that ſharp points having the moſt perfect communication with the earth, are not wholly exempt from receiving them. My firſt authority ſhall be Dr. FRANKLIN himſelf. "Let a perſon," ſays he, p. 60. "ſtanding on the floor, preſent the point of a needle at "twelve or more inches from it [the prime conductor], "and while the needle is ſo preſented, the conductor "cannot be charged, the point drawing off the fire as "faſt as it is thrown on by the electrical globe. Let it "be charged, and then preſent the point at the ſame diſ-"tance, and it will *ſuddenly* be diſcharged." The word *ſuddenly* means, I ſuppoſe, that it will receive an exploſion; that being the moſt natural and obvious proof of the *ſuddenneſs* of the diſcharge. The ſame thing is more directly aſſerted by Mr. HENLY, in vol. LXLV. of the Phil. Tranſ. p. 138. where he informs us, that in diſcharging three of his large jars, to the coating of which he had connected a wire nicely tapered to a point, the fire flew to the point, and the jars were diſcharged with a full and loud exploſion. A third, and equally deciſive, proof is furniſhed by Mr. NAIRNE's own experiments, though ſeemingly made with a contrary view. For when the double or interrupted conductor was uſed, and the ſecond conductor fixed down by ſcrews at about three inches diſtance from the firſt, the point preſented to the contrary end of the ſecond conductor was found to receive a ſtrong and loud exploſion, with a white light at the diſtance of at leaſt three inches.

If we compare this experiment with another, very common one, exhibited at the ſame time by Mr. NAIRNE, the compariſon will, perhaps, lead us to the diſcovery of a principle upon which electrical exploſions very frequently depend. Though the point, in the circumſtances above deſcribed, received ſo ſtrong an exploſion, yet when it was preſented directly to the prime conductor, it received no exploſion whatever at any diſtance, unleſs a ſucceſſion of weak ſparks, at the diſtance of about a quarter of an inch, can be called ſo. To what muſt this difference be attributed? Plainly to the different quantity of electric fluid accumulated on the prime conductor in the one and the other caſe. Where the point is preſented to the prime conductor, from the time the machine begins to work, the property which is attributed to them, and which, in ſome caſes, they really poſſeſs, of ſtealing away the electricity ſilently; this property, I ſay, operating from the very beginning, prevents the electric fluid from being accumulated in the prime conductor, and of courſe the quantity of it will always be ſmall. But when a double or interrupted conductor is uſed, the ſecond conductor receives no electricity till the prime conductor is pretty highly charged, and, if put at the greateſt ſtriking diſtance, not till it is fully charged, and conſequently the ſharp point preſented to the oppoſite end can carry away none of it till that time; when the whole quantity is thrown off at once. It ſhould ſeem then, that the exploſion in one caſe, and the non-exploſion in the other, depended wholly upon the different quantities to be thrown off: whence it will follow, that though a ſmall quantity of electricity will paſs off ſilently upon a point, yet that this power is very limited; for that if a ſomewhat greater quantity be applied ſuddenly to a ſharp point, it will not paſs off ſilently, but create an exploſion in proportion to its denſity.

The facts above related are a ſufficient anſwer to that other experiment of Mr. NAIRNE's, in which he exhibited a ſharp point, that when perfectly communicating with the earth drew off the electricity ſilently from the prime conductor; but received exploſions freely, when the communication was broken by interpoſing little iſthmuſes of ſealing wax. This experiment, it is true, demonſtrates that a broken communication will occaſion the ſharp point to receive an exploſion; and ſo far it muſt be owned to be concluſive. But if it was intended to ſuggeſt, that whenever ſharp points do receive an exploſion, it is owing to this circumſtance, and, conſequently, that Mr. WILSON's experiments at the Pantheon were unfairly made; in this view it has no weight, becauſe we have already ſeen that an interrupted communication is not the only circumſtance that will produce an exploſion, for that increaſing the quantity of electricity will have the ſame effect.

I cannot omit the opportunity here offered me, of remarking the unfairneſs of the inſinuations that have been thrown out to the prejudice of Mr. WILSON. Had there been any juggle in making his experiments, it would certainly have been detected by the committee appointed to examine them. And in caſe of ſuch a detection, it was the duty of the committee to lay open the impoſture both to the Society and the Public. Inſtead of which, inſtead of diſputing or even doubting the fairneſs of them, they have in a manner admitted it, by only ſaying in their report, that they appear to be inconcluſive. This, I ſay, is admitting the facts to be fairly ſtated: unleſs we could ſuppoſe their regard for Mr. WILSON, and tenderneſs for his reputation, had induced them, after detecting the fallacy of his experiments, to paſs it over in ſilence; of which improper partiality I do not know that they are ſo much as ſuſpected. What therefore the committee, after a ſtrict ſcrutiny of the matter, did not think themſelves warranted to ſay, I take for granted they would not inſinuate; and that therefore ſuch inſinuations can only ariſe from the levity of more obſcure perſons, puzzled perhaps by the ſeeming contradiction between Mr. WILSON's experiments and thoſe of Mr. NAIRNE, and too impatient to inveſtigate the real cauſes of that difference.

I am perſuaded, however, that the known property of ſharp points to carry off electricity ſilently, when the quantity is ſmall, together with that other principle, which I apprehend I have here eſtabliſhed, that they ceaſe to do ſo when the quantity is large; that theſe two (taken together) will clear up the whole difficulty, and account for Mr. NAIRNE's experiments, without any impeachment to thoſe of Mr. WILSON. I have already had occaſion, in the courſe of this argument, to conſider two of thoſe experiments, of which therefore I ſhall ſay no more; but proceed, without further digreſſion, to examine thoſe that remain.

The firſt I ſhall mention is that, in which the prime conductor, being previouſly charged with electricity, a ſharp point is preſented to it within the attracting, and without the exploding diſtance, and then brought ſlowly on towards it. In this caſe no exploſion follows; neither is there any reaſon to expect it ſhould, becauſe the quantity of electricity is gradually diminiſhed by the approach of the point, ſo that when it comes within the ſtriking diſtance there is not enough left to make an exploſion.

It is equally eaſy to explain what happens when a tranſverſe arm is hung ſo as to oſcillate freely upon the prime conductor, and two equal cylinders of tin-foil are ſuſpended, one at each end of this arm in perfect equilibrio with each other. The apparatus being in this ſtate, if the machine be worked, the two cylinders will remain ſtationary, neither of them aſcending or deſcending. They will alſo remain ſtationary, if a point be preſented to one, and a rounded end to the other. In the firſt caſe they are electrified, but remain motionleſs, becauſe there is no conducting body within the ſphere of their action. In the ſecond, the reſult is the ſame; becauſe, making in fact part of the prime conductor, the point preſented to one of them prevents any accumulation of electricity. When the point is withdrawn, and the rounded end ſuffered to remain, an accumulation takes place, becauſe there is nothing now to ſteal it away; and the conſequence is, that the cylinder deſcends towards the rounded end, and explodes as ſoon as that accumulation arrives at a certain period.

Thirdly, when an intermediate conductor is uſed, terminated at each end with a ball, and the middle of it reſting in equilibrio upon a pivot, on which it has a free oſcillation upwards and downwards; if in this ſtate a point is placed under the end moſt diſtant from the prime conductor, the machine being then worked, the other end will approach ſo near the prime conductor, as that the ſtream of electricity will flow freely into it, as faſt as it is produced by the action of the wheel. In this caſe there will be no exploſion; and the reaſon is obvious, becauſe the ſecond conductor, when it approaches ſo near the firſt as to form an uninterrupted channel for the electric ſtream, becomes virtually a part of the firſt. Hence the point operates upon both together, juſt as it does when preſented directly to the prime conductor, that is, it ſteals away the electricity by little and little, leaving not enough to give an exploſion. When inſtead of the point a poliſhed ball is placed under the ſame end as before, this being leſs diſpoſed to receive the electric fluid, conveys away none of it; ſo that accumulating to a certain degree upon the prime conductor, it explodes upon the contiguous end of the ſecond, which, having a free oſcillation, flies up with the ſtroke, and carries the oppoſite end towards the ball, where, being ſaturated, it gives a ſnap; the recoil of which ſnap throws that end up, and the contrary end back towards the conductor, and ſo on alternately, as long as the machine continues working.

The event, however, is widely different when the ſecond conductor, inſtead of having a free oſcillation, is ſcrewed down in one place, and at ſuch a diſtance from the prime conductor, as not to receive the electric fluid till conſiderably accumulated. For then the ſharp point, previouſly oppoſed to its other end, diſcharges it, as was before obſerved, not in a continued ſtream and ſilently, but at intervals, and with a ſtrong exploſion.

The laſt of Mr. NAIRNE's experiments, and the only one yet unconſidered, is that of the ſharp point, which, being fixed to a kind of inverted pendulum, oſcillated with great velocity under the prime conductor, without receiving any exploſion. Now from this experiment I do not comprehend how any general concluſion can poſſibly be drawn. It has been already ſhewn, from the acknowledgement of Dr. FRANKLIN, and the experiments of Mr. HENLY and Mr. NAIRNE, that electricity, accumulated to a certain degree, will explode upon a point. If, therefore, in any particular inſtance it does not explode, what can we infer from it, but that the accumulation in every ſuch inſtance was not ſufficiently great; which may happen either from the ſmallneſs of the apparatus, or from want of care in making the experiment.

And now if we look back upon Mr. NAIRNE'S experiments (which, by the by, have not all of them the merit of novelty) we ſhall find them to be nothing more than different exemplifications of this well-known principle, that ſharp points giving leſs reſiſtance to the ingreſs of the electric fluid will draw it off at a greater diſtance than blunt or ſpherical terminations, and where the quantity is ſmall will draw it off ſilently. This, I ſay, is the whole amount of his experiments; the only one of them in which the electric fluid had time to accumulate, being attended with a different event from the reſt, and producing, as might reaſonably be expected, a ſtrong explosion.

It is not, however, this ſingle property of ſharp-pointed conductors, which muſt decide the queſtion. We have already ſeen, that there are two properties inſeparable from them, both of which muſt be taken into the account, before we can determine the propriety of affixing them to buildings, particularly powder magazines, as preſervatives from lightning: firſt, their greater propenſity to admit the electric fluid, in conſequence of which they act upon electrified bodies at a greater diſtance than rounded ends will; and, ſecondly, their incapacity to draw away more than a certain quantity of electricity without an exploſion.

The firſt quality enables them, when electricity is accumulated gradually, or when they are brought gradually towards the electrified body, to ſteal away the fluid by little and little, till there is not enough left to give an exploſion. And hence, in common experiments, the point, placed at a greater diſtance than the ball, will prevent the electricity from exploding, as it otherwiſe would do, upon the latter. But if we combine this quality with the ſecond, the ſuperior propenſity to admit, with the incapacity in certain circumſtances of diſcharging ſilently, it will be evident, *à priori*, that the phenomena muſt in ſuch caſes be reverſed, juſt as they appear to be in Mr. WILSON's experiments; that the point muſt ſtrike at a greater diſtance, and the rounded end at a leſſer.

What puts this matter beyond a doubt is, that when the double or interrupted conductor is uſed, the experiment may be ſo managed, as that the ball ſhall receive an exploſion at a greater diſtance than the point, or the point at a greater diſtance than the ball, at the pleaſure of the operator. If care be taken, at the beginning of the experiment, to ſet the ſecond conductor at the greateſt

diſtance

diſtance from the firſt, compatible with its giving a full and ſmart exploſion, the point in that caſe will receive the exploſion at a much greater diſtance than the ball. I was myſelf once preſent at an experiment, when the difference was as 6 to 1, that is, the ball would receive no exploſion at a greater diſtance than $\frac{7}{8}$ths of an inch, when the point received it at ſix times that diſtance. On the contrary, if the ſecond conductor be put conſiderably within the diſtance above deſcribed, the ball will receive an exploſion much farther off than the point. Upon repeating both theſe experiments lately with a ſmall machine, I found the reſult to be as follows. When the diſtance between the firſt and ſecond conductor was 1$\frac{5}{8}$th of an inch, the point was ſtruck at 2$\frac{1}{16}$th of an inch; but a ball of 1$\frac{5}{8}$ths of an inch in diameter would not take the ſtroke at more than one inch. But when the diſtance between the conductors was only $\frac{5}{8}$ths of an inch, the point could not be ſtruck at more than $\frac{7}{8}$ths, whereas a ball of the ſame diameter as before was ſtruck at 7 inches and $\frac{3}{4}$, and a leſſer ball $\frac{3}{10}$ths of an inch in diameter at 6 inches and $\frac{3}{16}$ths. I have been told alſo, but have not yet had time to verify it, that a medium diſtance may be found, at which if the ſecond conductor be ſet, the point and ball preſented to the other end will be exactly upon a par with reſpect to the exploding diſtance.

Theſe phenomena, to perſons who have not carefully conſidered them, muſt appear ſo extraordinary, that unleſs the cauſe of the diverſity is explained, they will perhaps be led to ſuſpect ſome unfairneſs in making the experiment. The truth, however, is this; that when the two conductors are ſet at the greater of the two diſtances, the abſolute quantity of electricity collected before the exploſion is exactly the ſame in each experiment; and therefore the diſtances of the ball and point from the ſecond conductor being equal, and the greateſt at which either of them will be ſtruck, the exploſion will go to the point, as being more ſuſceptible, and giving leſs reſiſtance than the ball. But in the ſecond ſuppoſed caſe, when the ſecond conductor is ſet conſiderably within the former diſtance, the quantity of electricity which explodes upon the point and the ball is not the ſame; the point in this caſe exerting its known property of ſtealing away the electricity ſilently, which the ball from its greater reſiſtance is incapable of doing. The conſequence is, that the quantity accumulated to give an exploſion upon the ball is greater than that which explodes upon the point, and being greater will very naturally explode to a greater diſtance.

I might ſafely have reſted the matter upon this ground; but another proof, equally deciſive, having ſince occurred, it is but doing juſtice to my argument to inſert it here. In the experiments made with a view to ſettle this diſpute, and publiſhed by Mr. HENLY about four years ago, there is one, the fifth of that ſet, which it is difficult to reconcile with the doctrine here laid down, that electricity ſtrongly accumulated, and moving with great velocity, will explode upon a ſharp point rather than a ball.

He deſcribes it thus: "Having inſulated the jar, and "connected *by chains* with the external coating, on one "ſide a knob, and on the other ſide a ſharp-pointed wire, "both being inſulated and ſtanding five inches from "each other, I placed a large copper ball, eight inches "in diameter (inſulated alſo) ſo as to ſtand exactly at "half an inch diſtance both from the knob and the "point. The jar being fully charged, I delivered it upon "the copper ball by my diſcharging rod, whence it "leaped to the knob, which was three quarters of an "inch in diameter, and the jar was diſcharged by a loud "and full exploſion, and the chain was very luminous." Phil. Tranſ. vol. LXIV. p. 136.

It muſt be obvious to any careful electrician, and Mr. WILSON had in his anſwer obſerved, that an experiment thus looſely and unphiloſophically made, was not greatly to be relied upon; becauſe two chains being made uſe of, one to connect the ball, and another to connect the ſharp point with the coating of the phial, a different number of links, or different degree of tightneſs in the two chains, would produce a difference in the reſult: for it bing a known property of electricity to paſs moſt readily where it has the feweſt and the ſmalleſt intervals to leap over, the exploſion would naturally paſs that way, where the links were drawn tighteſt; or if both chains were left looſe, as from the plate they appear to have been, then where they were feweſt in number, inſtead of being determined by the circumſtances of bluntneſs and ſharpneſs.

It was however poſſible, that Mr. HENLY might be right in attributing it to the ſharpneſs of the point; and therefore, in order to ſettle this doubt, I deſired that the experiment might be tried over again in ſomewhat different circumſtances. Mr. CAVALLO accordingly tried it in the following manner. Upon an inſulated ſtand he placed a ball, about $\frac{6}{10}$ths of an inch in diameter, and a ſharp point, directly oppoſite to the place where a Leyden phial, when charged, was to be ſet down. Both of theſe, the ball and point, were connected to his diſcharging rod by copper wire. He then took the phial, which held about a quart, and having charged it, ſet it down before the ball and point, took his diſcharging rod, and completed the circuit. After the exploſion the diſtances were meaſured. In the experiments which I ſaw the phenomena were as follows: in the firſt, the diſtance of the ball was $\frac{19}{20}$ of an inch, that of the point $\frac{26}{20}$; in the ſecond, diſtance of the ball $\frac{16}{20}$, of the point $\frac{27}{20}$; in the third, diſtance of the ball $\frac{15}{20}$, of the point $\frac{26}{20}$; in the fourth, diſtance of the ball $\frac{18}{20}$, of the point $\frac{29}{20}$. In all theſe experiments the point, though conſiderably farther off from the coating of the phial, was ſtruck in preference to the ball. In a fifth experiment, when the point ſtood at double the diſtance, the ball was ſtruck, and not the point.

The reſult of theſe experiments being ſo widely different from the reſult of that made by Mr. HENLY, is a clear proof that he formed his concluſions too haſtily, having attributed to ſharpneſs and bluntneſs a phenomenon cauſed by the unequal reſiſtances of the chains. As this experiment may be made with almoſt any machine, thoſe who do not chuſe to repeat it will have no right to plead the want of a ſufficient apparatus, and muſt look out for ſome other reaſon to evade the force of it.

I come now to conſider more particularly the practical queſtion, whether the ſharp-pointed or the blunt conductors are moſt proper to be affixed to buildings, as preſervatives from lightning. And here it is neceſſary to obſerve, that buildings may be expoſed to a ſtroke of lightning in ſeveral different ways. The lightning which, to avoid prolixity, I ſhall only ſpeak of as poſitive electricity: the lightning, I ſay, may accumulate directly over the building; or it may be brought towards the building by a ſmall cloud fetching it in ſeveral ſucceſſive trips from a large cloud at ſome diſtance; or a large electrified cloud may be carried rapidly towards it by the wind: a circumſtance this by no means rare, there being no leſs than four inſtances of it upon record in the Phil. Tranſ. vol. XLIX. p. 16. and p. 309. vol. LXI. p. 72. and vol. LXIV. p. 351. In the firſt of theſe ſuppoſed caſes a ſharp-pointed conductor might poſſibly drain the cloud of its lightning as faſt as it began to accumulate, and thereby prevent any exploſion whatever. In the ſecond, as the cloud, by ſuppoſition, not being driven in one direction by the wind, could not move with any remarkable velocity, it is reaſonable to imagine, that in this caſe alſo there might be no exploſion; and that the electricity of the larger cloud might be gradually exhauſted. But if, according to the third ſuppoſition, a cloud of great extent and highly electrified ſhould be driven with great velocity in ſuch a direction, ſo as to paſs directly over the ſharp-pointed conductor, there can be no doubt but that ſuch a point, from its ſuperior readineſs to admit electricity, would take the exploſion at a much greater diſtance than a rounded end, and in proportion to the difference of that ſtriking diſtance would do miſchief inſtead of good.

But perhaps it will be ſaid, that every ſtroke of lightning falling upon a ſharp point is previouſly diminiſhed by that point, and therefore may more eaſily be tranſmitted through the conductor, than when it falls undiminiſhed upon a rounded end. Upon this ſuppoſition I muſt obſerve, that it not only contradicts Mr. WILSON's experiments at the Pantheon, but alſo Mr. HENLY's expement already referred to in this paper, where the fire flew to a very taper point, and melted the end with a ſtrong and loud exploſion. So alſo the ſharp-pointed conductors affixed in America to the houſes of Mr. WEST, Mr. RAVEN, and Mr. MAYNE, do not *ſeem* to have diminiſhed the force of the exploſion, if we may judge from the violence of its effects as related at large in Dr. FRANKLIN's works. It ſhould ſeem, therefore, that the power of diminiſhing a ſtroke, like that of preventing it, is only contingent, and depends, as we ſaid before, upon the degree of velocity with which the lightning moves.

The ſum of the whole is, that conductors, terminated by ſharp points, are ſometimes advantageous, and at other times prejudicial. Now as the purpoſe for which conductors are fixed upon buildings is, not to protect them from one particular ſort of clouds only, but, if poſſible, from all, it cannot ſurely be adviſeable to uſe that kind of conductors, which if they diminiſh danger on one hand, will increaſe it on the other. It is the duty of a pilot to keep out of the way of rocks; but it is alſo incumbent upon him, in avoiding the rock, not to take ſo large a compaſs as to run his ſhip upon a quickſand.

When I ſay that ſharp-pointed conductors may in ſome caſes diminiſh danger, I ſpeak of them, perhaps, rather too favourably: for their power of ſtealing away the electric fluid being confined to caſes where the accumulation is ſmall, it follows, that they only operate where their operation is not wanted. The caſes againſt which we wiſh principally to provide, are the exploſions of extenſive and highly electrified clouds; and here we have ſeen, that blunted ends, as acting to a much ſmaller diſtance, are entitled to the preference.

If it be admitted, that ſharp-pointed conductors are attended with any, the ſlighteſt degree of danger, how much muſt that danger be augmented by carrying them high up into the air, by fixing them upon every angle of a building, and making them project in every direction? Ought this to be adviſed while there is ſtill a doubt of the poſſibility of their doing miſchief? And can the committee, therefore, be perfectly juſtified for giving ſuch a decided preference to the uſe of ſharp conductors, in defiance of numerous experiments, not one of which they have attempted to controvert?

I have now done with the report of the committee, and ſhall next proceed to enquire whether, as ſome gentlemen apprehend, the termination of conductors is a matter of indifference: but this I muſt reſerve for the ſubject of a future paper.

APPENDIX.

I think it neceſſary to apprize the reader, that the foregoing remarks were drawn up before Mr. NAIRNE's paper appeared, and are therefore to be conſidered as relating only to his experiments.

I muſt at the ſame time obſerve, that, in ſtating the different circumſtances in which lightning may be collected ſo as to affect a building, I have ſuppoſed a caſe which poſſibly may never exiſt. I have ſaid, that lightning may accumulate directly over a building, and admitted, that in that caſe the cloud might be drained of it by a pointed conductor. Now we have no evidence, that the accumulation of lightning is confined to a ſingle cloud, or ſmall circumſcribed ſpot in the heavens. On the contrary, the numerous exploſions, which in moſt thunder ſtorms happen nearly at one and the ſame inſtant, rather lead us to imagine that a great part of the horizon is at thoſe times full of lightning, and therefore incapable of being drained. I would therefore wiſh to have my ſuppoſition underſtood as a mere imaginary ſuppoſition, for the ſake of rendering the argument more perſpicuous, and not as the admiſſion of a real fact.

Laſtly, I beg leave to correct an expreſſion I have uſed with reſpect to pointed conductors, that they only operate where their operation is not wanted. Now this is not accurately true: for if by operating upon a quantity of electricity too ſmall in itſelf to do miſchief, they prevent its growing to a great and dangerous quantity, this would, as far as it goes, be a very conſiderable advantage.

I ought therefore to have ſaid only, that pointed conductors afford no protection where the danger is great and imminent, and only obviate that which is diſtant and problematical; and that theſe laſt are not the caſes againſt which we principally wiſh to provide.

March 18, 1779.

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